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ACHIEVING "APPROPRIATE" LEVELS OF INVESTMENT IN  
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George C. Eads

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ACHIEVING "APPROPRIATE" LEVELS OF INVESTMENT IN  
TECHNOLOGICAL CHANGE: WHAT HAVE WE LEARNED?<sup>1</sup>

George C. Eads  
The Rand Corporation

This colloquium provides an opportunity to take stock of how far we have come since 1970 in answering a number of questions concerning the impact of investment in research and development on productivity improvement and what this relationship implies for government policy. Opportunities such as this occur all too infrequently. Typically in government, a "problem" is discovered, actions are initiated to "solve" it, and no retrospective look is ever taken to determine whether the "problem" still exists (or ever existed) and whether the "solution" was either effective, neutral, or counterproductive.

The 1971 colloquium did indeed highlight a "problem." Research that had been conducted over the preceding decade had shown that much of the observed increase in output per worker could be attributed not to increases in physical inputs but to something called improved productivity. There was a strong belief that a significant portion of this increase in productivity could, in turn, be associated with past investments in research and development. Studies had in fact revealed that specific investments in R&D often had very high pay-offs. Yet there were indications that private firms might systematically underinvest in R&D primarily due to their inability to capture fully the benefits generated thereby. It was concluded that if productivity improvements (and hence, economic growth) were to reach appropriate levels, government would have to step in to correct this tendency to underinvestment.

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<sup>1</sup>I would like to thank Arthur Alexander, Wendy Allen, Robert Perry, Dennis Smallwood, and Charles Wolf, Jr., for their helpful suggestions and criticism. The preparation of this paper was supported by The Rand Corporation as part of its program of public service. Additional support for its presentation was provided by the National Science Foundation.

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Prior to 1971 the government had not been inactive in supporting R&D. Government support of R&D, which had been both large and growing throughout the 1950s and early 1960s, peaked in 1967 and, between that date and 1971, *declined* in real terms at a rate of 4 percent per year. That was one of the primary worries that appears to have led to the convening of the 1971 colloquium. Further nonfederal R&D spending had continued to grow until 1969, but had also declined thereafter in real terms.

More blows were to come. In March 1971--one month before the colloquium--Congress killed the SST. This event signaled the coming of age of the environmental movement and the change in national priorities it reflected. In particular, it created a great deal of uncertainty about the future course of federal R&D support in this country.

If "underinvestment in R&D" was seen as the "problem," what was the proposed "solution"? After a brief flirtation with the idea of a massive program of direct governmental support for civilian R&D,<sup>1</sup> three relatively modest programs were set up--two within the National Science Foundation and one within the National Bureau of Standards. One of the NSF programs (the Experimental R&D Incentives Program--ERDIP) and the NBS program (the Experimental Technology Incentives Program--ETIP) were conceived as institutions that would initiate a broad range of small-scale experiments to determine the impact of various mechanisms the government might use to stimulate civilian R&D spending and the more rapid diffusion of technological innovation. The third, NSF's National R&D Assessment Program (RDA), was conceived as an institution to both sponsor and conduct studies aimed at advancing knowledge concerning such things as the impact of R&D

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<sup>1</sup>The documents justifying the various proposed projects were liberally sprinkled with the appropriate economic jargon--words like "externalities" and "multipliers." Among the most amusing of the ones I saw was a proposal to have the government subsidize the installation of two-way radios in trucks. Truckers would use these radios to report accidents and motorists in distress. It was claimed that absent such government support, the technology would never be developed, because truckers would never have an adequate incentive on their own to install radios.

spending on technological change, how the R&D investment decision is made within individual firms, the actual degree of underinvestment in R&D, and lessons for this country from foreign attempts to stimulate civilian-oriented technological change.

My remarks will be directed primarily to the work of the last of the three programs just mentioned, for it constitutes much of whatever advance in knowledge exists about the questions before us today.<sup>1</sup> I cannot claim to have reviewed all of this research, much less the larger body of work that has appeared since 1970 in the economics journals and elsewhere.<sup>2</sup> My remarks are to a degree impressionistic. I would welcome corrections from those who have been more diligent than I in following the literature.

A. *What More Do We Know About the Relationship of R&D Spending and Improvements in Productivity?*

By 1970 an impressive body of research had accumulated indicating that something called "technical change" or "technical progress" (i.e., something *other than* increased capital per worker) had accounted for the bulk of the observed increase in output per worker over the previous 70 or 80 years. The precise degree of importance of this factor and the precise means by which spending on research and development might eventually become translated into improvements in productivity (through being "embodied"

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<sup>1</sup>In February of 1976, the National R&D Assessment Program was reorganized and made a part of the NSF's Division of Policy Research and Analysis. ERDIP had all but disappeared much earlier. ETIP still exists. The Commerce Department is currently conducting a major evaluation of the program.

<sup>2</sup>For a more thorough review of much of the relevant literature, see Edwin Mansfield, "Federal Support of R&D Activities in the Private Sector," *Priorities and Efficiency in Federal Research and Development, A Compendium of Papers submitted to the Subcommittee on Priorities and Economy in Government, Joint Economic Committee*, October 29, 1976, pp. 85-115; Bela Gold, "Research, Technological Change, and Economic Analysis: A Critical Evaluation of Prevailing Approaches," *Quarterly Review of Economics and Business*, Spring 1977, pp. 7-29; and Richard R. Nelson and Sidney G. Winter, "In Search of a Useful Theory of Innovation," *Research Policy*, Vol. VI, No. 1, 1977, pp. 36-76, but especially pp. 36-46.



in new capital goods or through general, i.e., "disembodied," improvements in the state of knowledge) were matters of dispute. However, the importance to continued progress of maintaining high levels of R&D spending was not questioned.

Notwithstanding the proliferation of research results, I'm not convinced that we know much more today about these issues than we did in 1970. But the central issue is: What difference does it make that we don't?

I am skeptical of the value of pinning down more precisely the aggregate proportion of productivity change "explained" by technical change in its various forms. However, I am persuaded that we need to know more about how levels of R&D spending by one industry at one point in time eventually produce improvements in productivity and growth in output at a future date both in that industry and elsewhere. Without such knowledge it is impossible to know whether any direct or indirect governmental influence on such spending will improve matters, have a neutral impact, or make the situation worse.

In the work cited above, Gold suggests that we haven't improved our knowledge in this critical area because we have been trying too hard to generalize across industries. He believes that in our search for general relationships, we have missed the richness in the micro behavior which, in fact, provides the key to the puzzle. Gold even cautions against placing too much faith in the results that many of us have come to take for granted--that investment in R&D generates extremely high private returns, that concentration adversely affects the rate of diffusion of innovations, and so on.<sup>1</sup>

Nelson and Winter, while not taking so pessimistic a view about what we already seem to know, nevertheless argue that to proceed beyond our current (and, in their view, extremely primitive) level of knowledge about how investments in R&D affect differential growth rates in productivity by industry, we will not only have to focus our examination at a more micro level, but also completely change the perspective from which we view the process of innovation. They propose that this process (and, indeed, almost all microeconomic processes)

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<sup>1</sup>Gold, pp. 21-25.

be viewed in heuristic terms. They advance the concept of the "natural trajectory" as a factor explaining why certain patterns of innovation are generated.<sup>1</sup> They also set forth the notion of the "selection environment" as the factor that both largely determines the actual path of productivity growth generated by any given innovation and that strongly influences the kinds of R&D that firms and industry will find profitable to undertake.<sup>2</sup>

The suggestions of these authors reflect a growing awareness among researchers of the need for a much richer theoretical foundation for microeconomics; a foundation capable of dealing with a great diversity of observed behavior. This work also suggests that trying to fit behavior into many of the rigid categorizations that our current theoretical structure comprises may be counterproductive. Consider

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<sup>1</sup>Nelson and Winter, pp. 56-60. The authors provide the following explanation of the term (pp. 56-57):

In some of the writing on technological advance, there is a sense that innovation has a certain inner logic of its own.... [P]articularly in industries where technological advance is very rapid, advances seem to follow advances in a way that appears somewhat "inevitable" and certainly not fine tuned to the changing demand and cost conditions. Rosenberg talks of "technological imperatives" as guiding the evolution of certain technologies; bottlenecks in corrected processes, obvious weak spots in products, clear targets for improvement, etc..... These provide sharp signals that certain R&D projects are doable and are worth doing under a wide range of particular demand and cost conditions. Marginal changes in external conditions influence at most the ranking in terms of profitability of the set of projects associated with pushing technology in a given direction. We suggest that such *natural trajectories* are important, and can be studied. (emphasis added)

An interesting question which Nelson and Winter do not address directly is: What forces shifts in "natural trajectories"? Might not the findings of Jewkes, Sawers, and Stillerman stressing the critical role of individual inventors relate to the ways that new "natural trajectories" are discovered, while other results stressing the importance of large, organized R&D efforts actually refer to the ability to advance along "natural trajectories" once they are discovered?

<sup>2</sup>Ibid., pp. 61-70.

the case we are specifically concerned with here--the decision by an industry to devote resources to innovation. As Nelson and Winter observe, "... almost *any* nontrivial change in product or process, if there has been no previous experience, is an innovation."<sup>1</sup>

This tautological remark has powerful implications. For example, it causes Nelson and Winter to abandon a distinction that has characterized much of economists' previous thinking about innovation--the distinction between resource allocation decisions involving constant technology (i.e., movements along and between existing isoquants) and resource allocation decisions involving shifting technologies (i.e., movements between different production functions). It also renders obvious a proposition that previously has been largely ignored by scholars writing in this particular area of economics--that it is possible for an industry (or even a society) to *overinvest* in innovation.

More important for NSF, it complicates funding decisions. If in order to understand what has traditionally come to be known as the "R&D decision," it is necessary to understand *corporate* decision-making in general (and to do so at a very micro level), then a separately organized program of research support aimed solely at *R&D* decisionmaking may make little intellectual sense. Instead, NSF-sponsored research might more usefully be organized along industry lines with special attention given to industry interactions.

This is certainly consistent with recent experience in trying to disentangle the causes and implications for public policy of the materials shortages of 1973-74. Congress set up the National Commission on Supplies and Shortages to find out whether these shortages were one-time aberrations or whether they were harbingers of the future. One issue of special concern was whether there was a role for increased materials R&D.

In order to understand the origins of the shortages and to address such questions as whether the level of resources directed to materials R&D was "adequate," the Commission, of which I was

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<sup>1</sup> Ibid., p. 48 (emphasis added).



Executive Director, had to look closely at industry behavior and at the numerous influences that government exerts on it. Considering any one question or industry in isolation invariably proved futile--even counterproductive.<sup>1</sup> We concluded that the government lacks the capability to take the required detailed look despite the fact that decisions implying such knowledge are being made daily. Rather than recommend more spending on materials R&D, we urged that resources be devoted to creating the required analytical capability within government so that *all* decisions relating to industry (including whether additional materials-related R&D is really necessary) could be considered within their appropriate context.

What I wish to suggest is that if NSF really wants to advance our understanding of the "innovation process" significantly, it will have to be prepared to support research aimed at improving our understanding of industrial decisionmaking in general. Furthermore, this research will have to be industry- (or even firm-) specific, though considerable attention will have to be given to interindustry influences and to the effects of firm diversification. In effect, NSF will have to adopt the definition of "innovation" proposed by Nelson and Winter--the appropriate one, in my view.

B. *What Have We Learned About "Underinvestment"?*

In his assessment of the state of knowledge on the question of underinvestment as of 1970, Edwin Mansfield wrote:

Turning to the adequacy of the Nation's investment in research and development, there is too little evidence to support a very confident judgment as to whether or not we are underinvesting in certain types of research and development. However, practically all of the studies addressed to this question seem to conclude, with varying degrees of confidence, that we may be

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<sup>1</sup>See National Commission on Supplies and Shortages, *Government and the Nation's Resources: Report of the National Commission on Supplies and Shortages*, Government Printing Office, Washington, D.C., 1977, and associated appendix volumes.

underinvesting in particular types of R&D in the civilian sector of the economy, and estimated marginal returns from certain types of civilian research and development seem very high.<sup>1</sup>

Has our knowledge improved since 1970? To my mind, it has not. In his 1976 survey paper (a paper in which recent contributions were taken into account), Mansfield stated "... on *a priori* grounds, it is impossible to say with any reasonable degree of certainty whether there is an underinvestment in R&D in particular parts of the private sector."<sup>2</sup>

If our ability to produce definitive answers in specific situations has not improved (and I would agree with Mansfield that it hasn't), our understanding of the true nature of the "underinvestment" issue has deepened significantly. Furthermore, we are much better aware of how the issue must be approached if meaningful results are ever to be achieved.

In 1970 the question being asked was whether the *aggregate* level of resources directed to R&D was "adequate." We now correctly perceive the problem in much more microeconomic terms. We understand that the relevant baseline against which "adequacy" ought to be measured is not some abstract world of perfect competition and no government, but the real world of existing market structures and an existing complex web of direct and indirect government incentives.<sup>3</sup>

In this regard, it is particularly important to call attention to a largely post-1960s phenomenon--the rise of federal environmental, consumer product safety, occupational health and safety, and energy supply and use regulation. In the concluding section of this paper

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<sup>1</sup>Edwin Mansfield, "Contribution of Research and Development to Economic Growth of the United States," in *Research and Development and Economic Growth/Productivity*, National Science Foundation (NSF 72-303), Washington, D.C., 1972, p. 33.

<sup>2</sup>Mansfield, "Federal Support ...," p. 94.

<sup>3</sup>On this point see George Eads, "U.S. Government Support for Civilian Technology: Economic Theory Versus Political Practice," *Research Policy*, April 1974, pp. 2-16.

I will discuss this regulation in more detail. But for now it is sufficient to note that it has produced entirely new layers of incentives and avenues of influence, all of which bear on the "underfunding" issue.

Consider, for example, two important industries--automobiles and electric power generation. Both have been buffeted by a succession of events which, while reflecting demands that may be reasonable from a social point of view, nevertheless represent sharp breaks with the past. One characteristic of much of the recent spending on research and development in both industries is that it has focused on a series of crash efforts designed to produce quick fixes to constantly changing regulatory goals. For this reason, it is possible that the productivity of this spending *even in meeting the ultimate social goals to which much of it has been directed* has been quite low. On the other hand, new "natural trajectories" for the evolution of technology in both these areas seem to have been established. At least in automobiles, there are indications that foreign producers may have been quicker than domestic ones to sense this and to capitalize on it.<sup>1</sup>

Combine these regulatory impacts with the substantial increase in direct and indirect federal R&D support in both industries, and it becomes extremely questionable whether any residual "underfunding" remains. Indeed, considering the questionable efficacy of some of the stimulative mechanisms now being employed, and the powerful private incentives created by the recent massive shift in energy prices, I would rank these two industries high on the list for study as examples of federal *overstimulation*. As in the case I reported in my earlier paper--the commercial aircraft industry--attention

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<sup>1</sup> Compare this with Boretsky's explanation for the success of foreign automobile producers. See Michael Boretsky, "U.S. Technology: Trends and Policy Issues," Monograph No. 17, Program of Policy Studies in Science and Technology, The George Washington University, Washington, D.C., October 1973, especially pp. 91-94.

to these industries must turn away from the *level* of R&D spending and more toward its *composition* and *productivity* if useful adjustments in federal R&D incentives are to be made.

The troubling question of how to support basic research (or as Thurow<sup>1</sup> calls it, "support for basic capabilities") remains. The theoretical case for underfunding has always been strongest here. While there ought to be some better way to support our basic scientific and technological capabilities than merely to "[fund them] out of whatever mission-oriented R&D budget is popular enough to stand the strain,"<sup>2</sup> knowing exactly *how* to do this is a difficult problem. On the one hand, there are politicians who demand an immediate payoff from *any* expenditure of public funds. On the other, there are individual scientists and even agencies who concoct extravagant and unsupportable claims of benefits in an effort to obtain support. NSF should be particularly wary of joining in the latter group, even though it might seem politically attractive to do so. While the social benefits of particular *applied* research projects can sometimes be estimated,<sup>3</sup> those who attempt to make such estimates for basic research are engaged in an exercise that can only undermine their credibility.

C. *What Have We Learned About the Proper Role for Government in Stimulating Technological Change?*

In 1970, the consensus of researchers seemed to be that the government had an obligation to do *something* to increase the volume of resources directed to technological change. Many of the studies sponsored by RDA had as their aim to discover what that "something"

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<sup>1</sup>Lester Thurow, "The Relationship Between Defense-Related and Civilian-Oriented Research and Development Practices," in *Priorities and Efficiency in Federal Research and Development*, op. cit., pp. 6-9.

<sup>2</sup>This is also Thurow's characterization, *ibid.*, p. 7.

<sup>3</sup>Edwin Mansfield's recent attempt to do this ought to serve as a model. See Mansfield et al., "Social and Private Rates of Return from Industrial Innovation," mimeograph, September 1975.

might be.<sup>1</sup> ETIP and ERDIP were both expected to test specific intervention mechanisms--government procurement, support for venture capital, and so forth.

What has this effort taught us? Primarily, that it is much more difficult than we previously had thought to design constructive interventions. Different researchers have advanced different explanations for this difficulty. Roger Noll began the concluding section of the summary volume of his survey of government policies and their effects on technological innovation with the following words:

The main lesson to be learned from the existing literature is that numerous factors are likely to affect the innovative behavior of firms and the extent to which innovative performance, when guided only by private market decisions, is likely to diverge from economic optimality. Since the conditions in which firms operate vary with respect to these factors, the only firm conclusion about generalized, economy-wide policy is a negative one: no universal policy covering firms in differing market and technological environments is likely to lead to an efficient rate and direction of technological innovation.

Each industry operates in a particular environment with respect to market competition, the opportunities for technical progress, the extent to which nonprofit institutions are advancing the science upon which the industry is based, the appropriateness of new ideas, the expectations of each firm with respect to the likelihood of innovation in the industry, and numerous other factors that will affect its own program of research, development and market innovation. A rational government policy would be predicated on an investigation of the extent to which the conditions of particular industries were congenial to an acceptably efficient rate of technical progress. For each industry policy interventions would then be tailored to the specific aspects of its economic and scientific environment.<sup>2</sup>

<sup>1</sup>For a list of intramural and extramural research sponsored by RDA up to January 1976, see *Technological Innovation and Federal Government Policy: Research and Analysis of the Office of National R&D Assessment*, NSF 76-9, January 1976. This document also contains an interpretation of the results of the work that had been completed as of that date.

<sup>2</sup>Roger G. Noll et al., *Government Policies and Technological Innovation, Volume 1, Project Summary*, Division of the Humanities and Social Sciences, California Institute of Technology, Pasadena, p. 28.

On the other hand, Nelson and Winter believe the problem is more fundamental:

... [T]he hunt for appropriate policy instruments will not be an easy one. Macro measures will not do; thus proposals like a general R&D tax credit (which has been quite fashionable in the recent discussions in the U.S.) are beside the point. Policies need to be designed to influence particular economic sectors and activities. Regarding these, the key policy problem will be to augment or redesign institutions rather than to achieve particular resource allocations *per se*. Improving the railroads does not look like an objective that can be met through funding a few well specified R&D projects. Rather, the policy search must be for a set of institutions that will allocate resources appropriately over a wide range of circumstances and time.

... [T]he character of the appropriate institutional structure for the generation, screening, and effective exploitation of innovation depends on the underlying technologies, the nature of the demands for the goods and services, and the characteristics of the organizations supplying them. These critical variables differ from sector to sector. General analytic arguments, for example about externalities that are inherent in certain kinds of R&D, have little bite since they ignore sectoral differences. Useful analysis must focus on and illuminate these differences.<sup>1</sup>

Whether the answer lies in fashioning particular policy instruments to be applied in particular circumstances or in less attention to instruments and more to the institutional context within which technology-related decisions are made, the implication for the direction for future research is the same--an increased concentration on firm or industry decisionmaking. This does not mean that generalizations are impossible or, in particular, that work aimed at examining previously made generalizations should not continue. For example, the work sponsored by RDA on the impact of the antitrust laws on innovation has helped to put that issue into better perspective.<sup>2</sup> The work of ETIP in the procurement area has helped us to understand

<sup>1</sup> Nelson and Winter, pp. 40-41.

<sup>2</sup> Carole Kittl and Charles L. Trozzo, "The Effects of Patent and Antitrust Laws, Regulations, and Practices on Innovation," Institute for Defense Analysis, Program Analysis Division, 1976.

where procurement is likely to be effective and what the size of its leverage is likely to be.<sup>1</sup> What should be realized is that such research is likely to confirm what we already seem to be learning about how naive it is to expect easy solutions to difficult problems.

#### D. *Directions for Future Research*

I have suggested that there needs to be a fundamental shift in emphasis in NSF research support if we are to learn much that is useful about how government can effectively and appropriately influence the level of innovation. I do not pretend to have a "grand design" worked out, but let me speculate briefly on what might be done in an area of particular interest to me--the relationship between regulation and innovation.

For as long as 100 years, the activities of certain industries within our economy have been subjected to detailed regulation. This regulation has been imposed because of a belief that open competition would not produce socially desirable results. Either the existence of significant economies of scale would render survival by more than one firm so difficult that a "natural monopoly" would result, or entry would be so easy that "destructive competition" would occur thereby threatening the continued provision of essential services. Industries so controlled were called "regulated industries." As of the early 1970s their activities contributed about 10 percent of the gross national product. Most of the research concerning the relationship between regulation and innovation has been directed toward the activities of these industries.

The remainder of industry was classified as "unregulated." This was always something of a misnomer. The antitrust laws, securities laws, tax laws, and labor laws exerted a regulatory influence over the activities of firms in these industries. But, by and large, the foregoing distinction, based largely upon the

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<sup>1</sup>U.S. Department of Commerce, "Experimental Technology Incentives Program," mimeograph, September 15, 1976, pp. 10-14.

degree and type of control exercised by government, was both meaningful and well understood.<sup>1</sup>

As I have already noted, beginning in the early 1960s, however, a series of laws began to be passed that gradually extended government control over various aspects of industrial decisionmaking. In certain cases--for example, the Food and Drug Amendments of 1962--the mandate of an existing governmental agency was strengthened or broadened. However, most resulted in the creation of new regulatory bureaucracies, each with its own legislative mandate, each trying to achieve some social goal by means of direct control over some aspect of individual or firm behavior.

By the mid-1970s the two groups of industries had moved so close together that it was questionable whether it still made much sense to distinguish between them. True, there were still differences in degree. One important distinction lay in the degree of centralization of regulatory authority. For example, there was not an agency charged explicitly with regulating steel prices. But the combined influence exerted on domestic steel prices by the Council on Wage and Price Stability, the International Trade Commission, the Environmental Protection Agency, the Occupational Health and Safety Administration, the Equal Employment Opportunities Administration, and the Mine Enforcement and Safety Administration rendered this distinction more or less academic. (Indeed, it might be argued that the steel industry would have willingly given up its so-called "unregulated" status in exchange for consolidation of all the regulatory influences with which it had to deal into a single Steel Regulatory Commission.)

A systematic examination of the longer-term consequences--including the consequences for innovative performance--that may result from the transformation of the bulk of our industry from "unregulated" to "regulated" has yet to be undertaken. It is overdue.

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<sup>1</sup>This distinction carried over into the structure of the discipline of economics. Industries subject to detailed controls were studied in courses titled "Regulated Industries." Industries subject to the broader set of controls mentioned in the preceding paragraph were studied in courses titled "Industrial Organization."



Three interrelated areas of research, each of which addresses issues far broader than innovation, but without an understanding of which the innovation issue cannot be adequately treated, need to be pursued more or less simultaneously. The first would involve documentation of the shift. The second would explore its consequences for the behavior and structure of government. The third would perform a similar analysis for industry. Research in each area could begin with a modest program designed primarily to structure the larger research agenda. However, the relationships between the three areas should always be kept in mind and individual "linking" projects occasionally should be undertaken.

Research Area I--A Documentation of the Extent of Regulation of  
"Unregulated" Industries

It was claimed above that many ostensibly "unregulated" industries have within recent years become subject to a level of regulation that makes their former classification outmoded. One fruitful area for research would be to document this fact for a small but significant number of industries and, more specifically, to develop generalizable measures to characterize the extent of regulation to which such industries have become subject. Previously, an industry was considered "regulated" if a regulatory commission had been established to control it. There is no Steel Commission or Automobile Commission, nor is there likely to be. A more sophisticated means of describing the nature and extent of regulation needs to be developed.

Such measures, when added to those traditionally utilized by economists to characterize market structure, would provide a much more accurate picture of the decisionmaking environment within which given firms operate today than do structural measures by themselves. Their development might also eventually permit the development of testable hypotheses about how the degree of regulation--as well as the degree of competition--affects industry conduct and performance.

The effort described would clearly have to go far beyond a mere cataloging of applicable regulations.<sup>1</sup> It would also go beyond an attempt to calculate and compare their aggregate benefits and costs to either the industry or to the public at large.<sup>2</sup> Furthermore, the number of agencies with which an industry must relate, although perhaps an important variable, is likewise deficient as a descriptor.

Logical candidates for initial analysis would be steel, autos, plastics, paper, and electric power generation. The latter "traditional" regulated industry is included because one of the most important developments as far as these "traditional regulated" industries is concerned has been the proliferation of the number of regulations and agencies with which even they must deal.

#### Research Area II--Implications for Governmental Structure and Behavior

Within recent years, a debate has raged concerning whether this country should embark upon something called "planning." I suggest that this debate is academic--that the nation, through the regulatory apparatus it has created, already engages in "planning." Or, more accurately, the government has at its disposal a wide variety of means to control or influence industrial behavior and exercises these means. What it lacks is any way of monitoring the current and likely future condition of key industries and sectors, determining the likely short- and longer-run impact on this condition or individual regulatory actions (let alone the impact of a series of regulatory actions that might be considered simultaneously), spotting possible major conflicts in proposed regulatory actions, and harmonizing such actions in a way that eases these conflicts without seriously jeopardizing long-run social goals.

<sup>1</sup>For an attempt at this, see Council on Wage and Price Stability, *Catalog of Federal Regulations Affecting the Iron and Steel Industry*, December 1976.

<sup>2</sup>Such research is currently being funded by the Regulatory Research Program in NSF's Research Applications Directorate. See "Regulatory Research Program: Description of Program and Summary of Awards, 1975-1977," April 1977, NSF/RA-770019, pp. 11-15.

The primary objectives of the research in Area II would be to explore means within the *current* framework of government to achieve improved coordination, to examine previous attempts and why they have failed, and to investigate the possible longer-run implications should we fail to achieve this goal. Attention would have to be given to the political and social forces which have produced the expansion of regulation noted earlier<sup>1</sup> and the limitations of political institutions to control economic behavior.<sup>2</sup> Such work is obviously necessary if the path suggested above by Noll is to prove feasible.

### Research Area III--Implications for Industrial Structure and Behavior

To employ Nelson and Winter's terminology (correctly, I hope), the recent rise in federal regulation and, more specifically, the manner in which this regulatory authority is presently exercised, has radically changed the *selection environment* within which many important industries operate. The viability of the firms within these industries has come to depend as much (and, in certain cases, perhaps more) on the ability of their managers to understand, anticipate, and influence federal rules, regulations, and policies as on the ability of these individuals to deal with the actions of competitors and with other market forces. This change will, over time, fundamentally affect the type of individual who will emerge to manage such firms, the internal structure of the firms themselves, and, ultimately, the organization of the entire industrial sector of our economy. Not the least important consequence of this will be a shift in the firms' attitudes toward risk-taking in general and toward innovation in particular.

Certain research in the behavior of the traditional "regulated industries" may suggest what some of these consequences might be.

<sup>1</sup>On this point see Charles Schultze, "The Public Use of Private Interest," *Harper's*, May 1977, pp. 43-62, and James Q. Wilson, "The Politics of Regulation," in James W. McKie (ed.), *Social Responsibility and the Business Predicament*, Brookings, 1974, pp. 135-168.

<sup>2</sup>Schultze, *ibid.*, and Charles Wolf, Jr., "Implementation Analysis and the Theory of Non-Market Failures," preliminary draft, May 1976.

But much additional work directed explicitly at this question needs to be undertaken. This would be the primary subject matter of Area III.

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Although the preceding discussion of what we have learned since the early 1970s about the innovation process and what government ought to do to influence it might seem pessimistic, I myself am not pessimistic about the prospects of further constructive research. To be sure, we have not "solved" many of the "problems" that the 1971 colloquium highlighted, but we at least now know much better what the real issues are and how they ought to be addressed. That is no small accomplishment, and NSF ought to be proud for having had an important part in bringing this about.

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